

panied by a decided deficiency of the band on the green side, called by Piazzzi Smyth "the low sun band." Hence we have less red than usual and more green. This is due, in part at least, to the sunlight passing through a more than ordinary dense stratum of aqueous vapour, for we know that the thicker the stratum of vapour the more is the red light absorbed. But this is not all, for we have quite as much vapour without this green colour, but in these cases the sun, I believe, is not seen at all, but we get strips of green sky which are often seen. The atmosphere then, I believe, contains at present a large amount of vapour existing actually as vapour, and not condensed into clouds; hence even a great thickness of it is transparent except to those particular rays which aqueous vapour absorbs. The green colour can be seen only at a particular altitude, for only there is the thickness sufficient to produce the necessary absorption. At higher altitudes the peculiar pale silvery white is exactly what we are to expect.

Will you allow me to submit to the further consideration of the competent whether this phenomenon, seen at approximately the same time in Southern India, Ceylon, and the West Indies, could be due solely to the presence in the atmosphere of the vapour of water. Is not the air in these regions normally surcharged through a considerable period of every year with vapour of water? And yet not only is this an unusual appearance, but it has excited, wherever observed, both wonder and some alarm. In one respect the observation from Ceylon (*NATURE*, vol. xxviii. p. 597) is the most noticeable we have had yet, inasmuch as, even when the sun had attained "the very zenith," his light is said to have continued blue. My doubt is whether a phenomenon so rare could be due solely to a cause so everywhere common.

HENRY CECIL

Bregner, Bournemouth, October 22

P.S.—When Mr. Lockyer saw his green sun through the steam on the boat, were there not also mingling with the vapour sulphurous fumes from the funnel?

[The sun has been seen green through mist on the Simplon.—ED.]

Snake Poison

TOUCHING the effect of *Crotalus* venom on vegetable life, I am anxious to repair an error which appears on p. 552 of my work on "Snakes," where Dr. Mitchell is made to affirm that some healthy vegetables inoculated with the poison were "withered and dead next day, as if scathed by lightning." In some notes which I made many years ago on a too cursory reading of Dr. Mitchell's paper,¹ I omitted the inverted commas, which denote that the experiment was tried by Dr. Gillman of St. Louis, in 1854, but which Dr. Mitchell thought was too limited and wanting in detail to be of scientific value. I had overlooked Dr. Mitchell's comments and his own experiments on vegetable life, by which he was driven to the conclusion that the plants were injured by mechanical wounds, and not by the venom inserted into them. When writing my chapter under pressure of time long afterwards, I trusted too confidently to those careless notes, and to an impression gained through the old Virginia writers that venom is injurious to vegetable life.

But in a most interesting series of experiments twenty-five years ago Dr. Weir Mitchell found that the venom did not interfere, nor did it arrest alcoholic fermentation and its accompanying growth of sporules. To test it on the higher vegetable life he wounded plants in various parts of their stem and in various ways, taking three or four plants of similar size and growth—geraniums, tradescantia, and others—both succulent and of woody fibre, inserting venom into some and not into the others which were identical in character, and carefully noting the effects on each, which, for the most part, were similar in the inoculated and the merely wounded plants, the symptoms being such as were produced from the injury to the tissue, the leaf, or stem, as might be. "In many successive efforts to poison other plants with venom," says Dr. Mitchell in summing up the results, "*I failed in every instance.*"

A more careful perusal of Dr. S. W. Mitchell's paper now enables me to offer this explanation of the misrepresentation of

¹ "On the Venom of the Rattlesnake," by Dr. S. Weir Mitchell, "Smithsonian Contributions to Knowledge," vol. xii. 1856. Washington, D.C., United States.)

those exceedingly interesting experiments, fully detailed in vol. xii. of the "Smithsonian Contributions."

Cleveland, Ohio

CATHERINE C. HOPLEY

Simultaneous Affections of the Barometer

MY thanks are due to Dr. Balfour Stewart for his kindly pointing out that simultaneous movements of the barometer, like those I had described in my paper of January last, and also in the "Brief Sketch of the Meteorology of the Bombay Presidency in 1881," written in August, 1882, were first observed by the late John Allan Broun. Owing to my connection with meteorological work being short—of only fourteen months' duration—my attention had not before been drawn to this fact. It is to me interesting to learn also that the late John Allan Broun considered that there was a connection between these movements and the earth's magnetism.

The *Proceedings of the Manchester Literary and Philosophical Society* for the last few years do not appear to have been received in Bombay, but they have now been applied for.

A. N. PEARSON

Meteorological Office, Bombay, September 14

Table of Different Velocities

IN reading over the interesting table of velocities drawn up by Mr. James Jackson, and published in *NATURE* to-day (p. 604), there is one item omitted, which the author may like to add to his list, viz. the rate at which detonation travels, as exemplified by a train of compressed gun-cotton. This has been computed by Abel and Nobel to be between 17,000 and 19,000 feet per second, or rather more than 200 miles in a minute. In Mr. Jackson's table, therefore, the detonation of gun-cotton would come in somewhere between the velocity of sound in water and the velocity of electricity.

H. BADEN PRITCHARD

Woolwich, October 18

OSWALD HEER

WE briefly announced last week the death, on September 27, at Lausanne, of Dr. Oswald Heer, Professor of Botany in the University of Zurich, aged seventy-four years and twenty-seven days. He was born at Nieder Uzwyll, Glarus, Switzerland, August 31, 1809. His whole mind seems to have been imbued from an early age with an intense love of nature, and his devotion to it led him to prefer its study to the discipline of the Church, which he had entered. Heer's early reputation was made as an entomologist, and from 1834 forwards he published many works and papers on entomology, chiefly on Swiss insects, and more especially on Coleoptera, most of which treated exhaustively on the vertical distribution of species in the Alps. Possibly he is best known (as an entomologist) in this country by his monographic work on the beetles of Switzerland, which appeared in 1838–41. In this work he did for the Coleoptera of that country what Frey has more recently done for the Lepidoptera, but, of course, lapse of time has rendered Heer's labours out of date as compared with Frey's. This monograph appeared in two forms, but that which is best known was styled "*Fauna Coleopterorum Helvetica*," and extended to over 600 pages. But his attention was soon attracted, perhaps by some fortunate chance, towards the remains of plants which were being disinterred from the Tertiaries to the north of Lausanne and elsewhere on the Lake of Geneva, and his whole energy became absorbed in unravelling and restoring the vegetation of the past, and continued so until the close of a laborious life. In 1855 appeared the sumptuous "*Tertiary Flora of Switzerland*," a work which at once placed him in the first rank as a specialist; and being a prolific and imaginative writer, untiring industry, he has since contributed to palaeontology a nearly uninterrupted series of works on his favourite subjects, terminating but last year with the sixth volume of the "*Flora Fossilis Arctica*." Few earnest workers have lived to see their work more highly appreciated, and the gratification he must have felt at the sub-

stantial honours that were showered upon him for many years, from grants of money to honorary distinctions of the highest order, must have gone far to compensate for a malady which had for several years left him bed-ridden. The high reputation he so suddenly acquired, more especially in England, was doubtless mainly due to the friendship of Sir Charles Lyell, who constantly quoted his works, almost to the exclusion of those of other writers on similar subjects. His quickness in seizing the characters of even fragments of fossil leaves, his aptitude in describing them combined with the boldness of his inductions and a certain grace of diction, centred attention on his work, and unconsciously diverted it from his eminent contemporaries, Unger, Goeppert, Saporta, and Ettingshausen. The place he occupied was unique, and his opportunities were proportionally great; his loss will be felt, for it will be difficult to find workers, as competent, either able or willing to dispose with such rapidity of the constantly increasing material brought from distant, and especially Arctic, expeditions.

The subject which he had thus made his own is one of exceptional difficulty, both from its wide range and the character of the material to be dealt with, and the path which he trod with such assured steps will be trodden by others with doubt and hesitation. If imitation is the sincerest form of flattery, then was Heer most amply flattered, for nearly all works on the newer fossil floras have been modelled on his bases, and he has become the founder of a school which bids fair to monopolise for some time to come this branch of palæontology. It is no disrespect to the dead to mention the open secret that nearly all English botanists, and very many geologists, have doubted the possibility of determining, except in rare instances, the detached and broken leaves which make up nine-tenths of the Tertiary floras. It is fortunate that Heer's temperament was sanguine, and his belief in his power to interpret the material unlimited, else the marvellous Tertiary floras from the Arctic circle, which so profoundly exercise the imagination, would have remained a sealed book. His powers are the more surprising, as his health does not appear to have permitted much travel, a winter spent on the cultivated side of Madeira seeming to have been his only actual acquaintance with extra-European floras. Much of his work, too, was produced under conditions the reverse of favourable for exact determination and comparison: a friend relates that when calling to convey one of the numerous awards made to him by English scientific bodies, he found the Professor lying down with a small table arranged to cross the bed, upon it being specimens which he named while an assistant made drawings.

Besides the fossil floras and insect faunas of his own country, his works comprise, among many others, descriptions of the Carboniferous, Jurassic, Cretaceous, and Tertiary floras from round the Arctic circle, and from Germany, Austria, Italy, Portugal, and even more distant countries. In 1861 he was invited to England to describe the Tertiary flora from Bovey Tracey, and a work appeared upon it in the *Phil. Trans.* of the following year. It is certainly strange that this and nearly the whole of the fossil floras containing dicotyledons examined by him have been referred to the Miocene age, and certain prevailing types seem to recur in the greater part of them; but it is not our province to discuss the correctness of these views here. He was much in earnest and zealous in the extreme, and the importance and value of his work, including as it does figures and descriptions of species which may almost be numbered by the thousand, is incontestable. So much accomplished, in spite of ill health and probably with less extensive herbaria to consult than are available in this country, commands respect; and however the study of fossil plants may rank in the time to come, Heer's name will for ever be bound up with it as its great pioneer.

THE BACKWARD STATE OF CHEMISTRY IN ENGLAND

IN the address of the President of the British Association last year the report as to progress in one of the principal Sections, that of Chemistry, is certainly a very meagre one. It is indeed confined to a general statement of the value of materials derived from coal and coal-tars, &c., and cannot, strictly speaking, be termed chemical. Again, in the address of the President of the Chemical Section, the existence of such a branch or division of chemistry as that termed "organic," and in which more perhaps has been done during the past twenty years than in mineral chemistry during the century, is completely ignored. And unfortunately the reason does not seem far to seek, for very few of the papers presented to the section had direct connection with the chemistry of carbon.

But it is not only at the British Association meetings that this neglect of organic chemistry occurs, but even at the Chemical Society itself the number of contributions to this section of chemistry is very small. In 1881, out of more than eighty communications to the Society only about thirty are relating to carbon compounds. In 1882 the proportion is somewhat greater—thirty-one out of sixty-five. It would be perhaps very unfair to institute a comparison between our Chemical Society and a much younger one, that of Berlin, as they are somewhat different in constitution: and the feeding ground, if it may be so called, is more extensive in the one case than in the other, but still the disparity in number of papers is scarcely to be accounted for in this way.

Chemistry generally, and especially the so-called organic chemistry, appears to have been very much neglected for some years past in this country.

The cause of this lagging behind, especially in a science of such infinite practical applications as chemistry, by this country is somewhat difficult to understand.

We certainly have not the number of chemical schools in England as in Germany, but making allowance for that and comparing the past decade in the two countries we appear to be grievously behind, both in number of investigations as well as in their theoretical or practical importance.

The cause can scarcely be attributed to any want of energy or appreciation of the value of research on the part of our manufacturers, for they are in many instances obliged to seek assistance out of the country. It seems to some extent rather to be owing to a non-appreciation of the science by the general public, although in its more elementary stages it is more extensively taught than in any other country, and this non-appreciation reacts injuriously on the schools themselves. Although such an intensely practical science and capable of such varied applications, the chemical investigator must pass over many weary stretches of complex and to all appearance purely theoretical and useless work before reaching a brilliant practical result.

Unfortunately, until quite recently but few of our schools were so constituted and equipped that a student might work on anything like equal terms with his fellow in a German school.

Now, however, we have a goodly number of chemical schools rising up, with in many cases professors trained in German laboratories. But in many of these the professor labours under the great disadvantage, not only to himself and the science, but to the students, that he has too much mere routine teaching and too little time for that original work, or research, which acts so powerfully in encouraging and stimulating his students to get more than a mere insight into the working and mechanism of the science, and become investigators themselves.

The cost of working in an English laboratory is somewhat greater than in a German one, but this difference is